For the monument near Nymphæum I propose the name of Assyro-Pseudo-Sesostris, so as to preserve the legend of Herodotus, and at the same time to distinguish its class.

It is worthy of remark that the nearest Assyrian monuments accessible to the West are in the immediate neighborhood of Smyrna, and that they must possess an antiquity of some three thousand years at least.

Smyrna, June 17th, 1865.

IV. Reply to the Strictures of Prof. Weber upon an Essay respecting the Asterismal System of the Hindus, Arabs, and Chinese.

BY PROF. W. D. WHITNEY.

Presented to the Society Oct. 11, 1865.

Prof. Weber of Berlin, in the ninth volume of his Indische Studien (pp. 424–59), has replied to my review and criticism, published in the first part of the eighth volume of the Society's Journal (above, pp. 1–92), of his opinions touching the character and origin of the Hindu, Chinese, and Arab systems of lunar asterisms. Although his paper has an interest and bearing chiefly personal, hardly changing at all the scientific aspect of the questions discussed, I am not willing to let it pass altogether without rejoinder, since it charges upon me a misrepresentation of his views and arguments in certain respects; and especially since, in at least one important respect, I am obliged to confess the charge well founded, and have to excuse and apologize for my error.

In my former paper, namely, I ascribed to Prof. Weber the confident belief that the Chinese and Arab systems were, both of them, immediately derived from the Hindu. Herein, as must be frankly acknowledged, I misstated the position held by him as to the Chinese system, treating as a positive dogma what he presented only as a questionable, though probable, theory. This was an oversight on my part which I much regret, and which justly exposes me to censure. But I may, I think, be allowed to plead, in mitigation of my offense, that I have not, after all, done Prof. Weber's argument any real injustice—nay, that I have even done it better justice than it receives at his own hands. His most important thesis, with the establishment of which his second essay is chiefly occupied, is this: that "the *sieu*, in respect of order, number, identity of limiting stars, and inequality of distance, correspond to one of the most modern phases of the Hindu *nakshatras*, prior to which these latter have their own peculiar history of development." (Essays on the Nakshatras, i. 285.) Now if this thesis is proved, as Prof. Weber claims, I see not how he or any one else can for a moment hesitate to believe that the *sieu* are a derivation from the *nakshatras*. If

an institution has passed through a succession of phases in the hands of one nation, and is found in the possession of another in a form corresponding with the last of those phases, it must be very positive and unequivocal evidence which shall have the right to convince us that the latter nation did not borrow it from the former at the end of its history of changes. But the opposing considerations which Prof. Weber suffers so to weaken in his mind the force of his own argument that he is afraid to adopt its legitimate conclusion, are really of no weight whatever as opposed to it: they are, in part, "the incongruencies upon which Biot lays such stress"—which incongruencies, as I have shown in my former essay, have no existence save in Biot's misapprehensions of the Hindu system—and, in part, correspondences and differences among the members of the three systems which would be without difficulty reconcilable with the theory of derivation from India to China, if only there were to be found elsewhere reasons for believing in the fact of such derivation. I do not see, therefore, that any of my counter-argumentation is rendered unnecessary by Prof. Weber's disclaimer of the belief which I mistakenly attributed to him; but only that I ought to have directed it, not against his personal opinion, but against the opinion which ought to be arrived at and confidently held by everybody whom he shall succeed in persuading of the truth of his principal thesis.

Nothing which my essay contained tended, so far as I am aware, to deny that Prof. Weber held Babylon to be the ultimate place of origin of the lunar zodiac; it was only the strange and unnecessary complication of his view, as I understood it—that, while Babylon was the ultimate source, neither Arabia nor China had derived it from Babylon directly, but each had gotten it indirectly, through India—which I set myself to oppose.

As regards the immediate origin of the Arab *manâzil*, Prof. Weber still asserts with the utmost confidence, against my objections, that they must be looked upon as imported out of India into Arabia. The only new evidence bearing upon the question is that contained in a paper by Steinschneider in the Journal of the German Oriental Society for 1864 (vol. xviii., pp. 118–201), respecting which Weber claims that "it leaves hardly a doubt about the matter." I am sorry to say that I cannot in the least concur in this estimate of the results of Steinschneider's researches. In the first place, so destitute is his paper of unity and method, of such a heterogeneous and confused mass of notices, references, digressions, and by-the-way's, is it made up, that, for myself, I hardly know what it does and what it does not attempt to prove—nor was its author, at the end of his work, in a better plight: he most ingenuously confesses that he cannot quite see what he has been driving at, or to what conclusions his wanderings have led him.* But in the second

* The passage deserves to be quoted, as one of the curiosities of literature. "It would now have been my task to put together in a brief form a concluding *result*, at least respecting the chief points. I must, however, acknowledge to my sorrow, that the reduction of the new and in part perplexing material, in the midst of various outward interruptions, and *during the printing* of the essay—which for obvious considerations I might not disturb or delay—has not permitted me the repeated

place, the authorities upon whom Steinschneider and Weber rely to prove that the Arabs acknowledge themselves to have borrowed the asterismal system from India, have no right to be heard at all upon the point here in controversy. They are *savants*, great lights of the period of Arab literary and scientific culture, the oldest of them writing at a time some century or two posterior to the rise of Islam; and the *manâzil*, on the testimony of the Korân itself, are an ante-Islamic institution. No one who bears in mind the deep obscurity resting upon Arab conditions prior to the time of Mohammed, the paucity of authentic information respecting them collectable from the Arab historians, will be likely to believe that Jafar and al-Kindi are entitled to speak with authority respecting the origin of a system whose use dates back to primitive times in Arab history. The late lamented Woepcke, in whose learning and critical judgment we have reason to repose the highest confidence, is unwilling (in his last communication to the Journal Asiatique) to believe upon the sole testimony of Arab mathematicians that the *gobâr* signs of notation, even, were brought to Arabia from India; pointing out that "unfortunately, historical criticism is wanting to such a degree in most Arab writers, that their evidence can only be accepted with the greatest reserve, when it concerns facts of which they could not have immediate and certain knowledge." (Journ. As., [6] i. (1863), p. 69.) We do not need, however, to discredit entirely the statements of the Arab scientists as to their borrowings from India in connection with the asterismal system; we only need to interpret them by the aid of other known facts, and to find out what they really mean. And what are the facts? In the first place, the Arabs had a system of lunar asterisms before the rise of Islam. In the second place, a hundred years after Mohammed, in the eighth century, it is well established that the Hindu astronomical science, as represented to us by the Siddhântas, and known to date from the fifth and sixth centuries, was brought to the knowledge of the Arab learned, and eagerly accepted by them; and, in the following century, we find them ascribing their doctrine of the asterisms to Hindu authorities. If, now, their later series of asterisms agreed precisely or very closely with that which the Siddhântas accept and teach, we should have a right to conclude that they actually obtained them from the Hindus, abandoning or modifying (to an extent which

working-over, and the quietness of thought, requisite for the accomplishment of such a task. Let, then, the following corollaries be received with indulgence. They are meant to serve only as an invitation to the examination and comparison of materials inaccessible to me, and so to lead the way to a definitive opinion." (p. 200.) Of the corollaries thus modestly and provisionally set up, the only one bearing on the question now under discussion is to the effect that, according to the Arab authorities, the Hindus counted twenty-seven asterisms, instead of twenty-eight: and this, as we shall see farther on, makes directly against the theory that the Arab system is derived from India. If, now, Dr. Steinschneider has later gained confidence to say "I claim, as the main desert of my essay, the proof that the lunar stations belong to the grand circle of ideas which have come *forth from India* to Europe, and have swayed the Middle Ages" (see Weber's essay, p. 427, note 2), we can only reply, that even his former confusion of mind was greatly preferable to a conclusion so little warranted by his facts, and which exaggerates the European influence of the lunar zodiac as much as it over-estimates the value of the evidence showing India to be its original home.

it would always remain beyond our power to determine) their own ancient institution: we could say with confidence that the *manâzil*, as we know them, were derived from the *nakshatras*. But, so far is this from being the case, that the Arab series corresponds with the Hindu in only two-thirds of its members, while, in a considerable part of the remaining third, it agrees closely with the series accepted in far-off China. Moreover, the Arabs never think of counting less than twenty-eight asterisms, while the Hindus, for the purposes of astronomical and astrological calculation, almost uniformly acknowledge only twenty-seven. The case being thus, it is, I submit, incomparably the more plausible supposition that the later system of *manâzil* is the same with the earlier; that the Arabs did not servilely abandon their own time-honored institution and put a foreign one in its place; and that, when they confess their indebtedness to the Hindus, it is for the scientific application of the system, for its astronomical and astrological uses, which they would naturally adopt along with the rest of the scientific Hindu astronomy. They might truthfully ascribe their doctrine of the *manâzil* to India, even while they adhered strictly to every one of the familiar constellations which their fathers had been wont to observe and revere.

I can discover, therefore, no ground whatever for the assumption that the known *manâzil*, considered as a series of asterisms ("with Sharatân at their head," as Weber repeatedly specifies—as if the choice of a starting-point, in an annular series, was a matter of other than wholly subordinate consequence, or could determine the identity of the system), have been modified by post-Islamic borrowings, or that they are anything but the ancient *manâzil* which the Korân mentions, and whose origin goes back to a period unattainable by the knowledge or the surmises of Arab *savants* of the ninth century. Of course, the possibility of their ultimate derivation, after all, from India is not excluded; but no evidence has been yet adduced which goes to prove it.* To our apprehension, the Indian, Arabian, and Chinese series of asterisms must remain, for the present, three independent forms of the same system, and their historical tie of connection is yet to be discovered.

In my former article, I have charged Prof. Weber—not, indeed, with holding that the *nakshatras* are single stars only, but—with reasoning about them, in one important respect, as if they were single stars: by regarding, namely, the spaces in the heavens which they designate, and to which they give names, as measured from one asterism to the next, thus making them virtually stars limiting the spaces, like the Chinese *sieu*, instead of groups occupying the spaces, like the Arab *manâzil*. Herein he claims that I have misrepresented and done him flagrant in-

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* Prof. Weber, in his eagerness to admit the Hindu derivation of the *manâzil*, is ready to accept, as indication of a possible early astronomical influence of India on Arabia, "Levy's discovery of an inscription in ancient Hindu characters on the peninsula of Sinai;" but, aside from the infinitesimal value of such a fact, even if established, in such a connection, the alleged discovery is, as yet, only a conjecture: Levy has found among his Sinai inscriptions one of which he can make nothing; but, confident that he perceives in its characters a likeness to ancient Indian alphabets, he turns it over to the Indianists, for them to read it—if they can. (See Zeitsch. d. Deutsch. Morg. Ges., xiv. (1860) 483.)

justice, the plain purport of his words being otherwise.—Let us look at the facts in the case.

Biot has found, in the eighth chapter of the Sûrya-Siddhânta, a definition of the places of the junction-stars of the several asterismal groups which shows them to stand at greatly varying distances (from three to thirty degrees) from one another. These distances, or intervals from star to star, he regards as constituting, after the manner of the Chinese *sieu*, the lunar mansions or stations into which the moon's path is meant to be divided; and he declares them, on account of their inequality, so ill-suited to their purpose that the incongruency constitutes a powerful evidence of the non-originality of the Hindu system. I have repeatedly pointed out—what, indeed, must be evident to any one who has examined the Sûrya-Siddhânta with any care—that this is a palpable misapprehension on the part of Biot; that the treatise referred to does not, any more than any other Hindu authority, measure the *nakshatra*-spaces by the mutual distances of the *nakshatra*-groups, but divides the ecliptic into twenty-seven equal parts; the definition of position of the junction-stars being made for a wholly different purpose. Prof. Weber, on his part, fully accepts and endorses Biot's error, and the mode of division implied in it;* this inequality of distances among the *nakshatras* is to him (Nakshatras, i. 285) one of the characteristics of that late phase of development of the Hindu system to which the Chinese corresponds; and he promises to prove that in the Brâhmaṇas is to be found "no trace of an inequality of distances; but, on the other hand, traces and direct notices which point to their equality." Who, now, would fail to draw from such a concession and reply the conclusion that Weber, like Biot, measured the *nakshatra*-spaces from star to star, and to the objection raised by the latter could only oppose the plea that a different series of asterisms was recognized in the olden time—proceeding afterward to fortify his plea by elaborate arguments, founded on the names, numbers, and divinities of the asterisms, to the effect that the series had been a variable and shifting one? Again, later (Nakshatras, i. 314), he points out the discordances between Brahmagupta and Varâha-mihira, touching the dimensions allotted by them to some of the asterismal spaces, as a very strange circumstance, and one which "seems not to furnish the most favorable testimony to the accuracy of identification of the *nakshatras* in the sky at the period." Here, again, one cannot but ask, how should such discordance imply inaccuracy of iden-

* That he should have done so in his Essays on the Nakshatras is not much to be wondered at; but I totally fail to comprehend how he could have repeated the blunder in his later article (p. 440), after I had so fully exposed its character (above, pp. 18, 21)—at least, without attempting to controvert my position, and to show that Biot had understood, and that I had misunderstood, the Sûrya-Siddhânta. The matter is, indeed, too clear for controversy: the Sûrya-Siddhânta does *not* teach an unequal division of the zodiac; its positions of the junction-stars, laid down in the eighth chapter, are in no wise inconsistent with the system of equal divisions inculcated in the second; nor do they constitute a special coincidence between a later Hindu form of the institution and the Chinese *sieu;* and for a charge of incongruency they furnish no manner of foundation. As well infer from an astronomical definition of the places of α Arietis, α Tauri, α Geminorum, and so on, that the signs of the zodiac are of unequal extent.

tification of the star-groups, unless the spaces be measured from star to star? If each asterism has a space set apart to it simply from the neighboring portion of the ecliptic, authorities may well enough differ as to whether equal division shall be made of the interval between two asterisms, or whether a larger part of it shall be attached to the one or to the other of them. And then Weber goes directly on to remark the inconsistency between the spaces assigned by the two authorities referred to and those derivable from the Sûrya-Siddhânta, the latter being measured, according to Biot's erroneous method, by the intervals of longitude between junction-star and junction-star: and he does not give us the slightest reason to suspect that he contemplates any difference in the mode of measurement in the three several cases.

I do not need to take up in detail other like instances; so far as I have been able to discover, the Essays contain nothing from which a conclusion could legitimately be drawn militating against that derivable from the passages already discussed: everything, rather, tends to support the latter. Indeed, I conceive my understanding of Weber's meaning to be amply justified by the very quotations of his former words which he makes in his reply, for the purpose of convicting me of misrepresentation. In every instance, he speaks of the equal or unequal "distances," or "mutual distances," of the *nakshatras*, or the lunar stations. Now, as everybody acknowledges that the word *nakshatra* has two meanings, namely 'asterism,' and 'space in the sky, or division of the ecliptic, marked by an asterism,' it seems to me not only a natural, but an unavoidable inference, that when any one talks about the "mutual distances of the *nakshatras*," he means the intervals between the asterisms themselves, the stars or star-groups—just as, when one speaks of the mutual distance of Rome and Naples, he is necessarily understood to refer to the interval between the two cities, and not to the respective extent of the Roman and Neapolitan territories. Who, of his own head, could possibly have imagined that Prof. Weber would habitually write of the "mutual distances" of the successive divisions of a circle, between which there are no distances—which are conterminous? If he had usually talked about "equal spaces," or "equal extent of the *nakshatras*," he would at least have given us a clew, by which we might have arrived at a recognition of what he at present claims to have been the true intent of his language everywhere. But, in the solitary passage which he is able to cite where he uses the expression "the *spaces* of the twenty-seven *nakshatras*," he immediately adds "their mutual distances from one another," as if expressly to guard against our understanding the "spaces" to be measured otherwise than by the "distances" between the stars!

If Prof. Weber, accordingly, now maintains that, whenever he said "equal or unequal distances of the *nakshatras*," he all the time meant "equal or unequal extent of the *nakshatra*-spaces," measured not by interval but by vicinity, it is reasonable that we should believe him upon his word: but it is equally reasonable that he should allow the misunderstanding to have been his own fault, and not imputable to us, who were obliged to infer his meaning from his language alone.

I am, I must confess, not a little puzzled to understand upon what

ground it is that Prof. Weber (p. 437) pronounces my habitual representation of the word *nakshatra* by 'asterism' to be, "etymologically, thoroughly unjustifiable." The absolute derivation of *nakshatra*, indeed, still remains a mooted question, as at the time when our notes to the Sûrya-Siddhânta were prepared; neither Aufrecht's explanation of it (*nakshatra* = *nakta-tra*, 'night-keeper'), nor Haug's (*nakshatra* = *nak-sattra*, 'night-station') being wholly satisfactory and convincing—especially the latter, which is directly opposed to the demonstrably earliest meaning of the word. This, as Prof. Weber himself has been at much pains to show (Nakshatras, ii. 268 sq.), is 'star' or 'constellation.' Nor has the term ever lost its primary signification, exchanging it for that of 'space in the sky;' the latter has only been added as a recognizably secondary or derived meaning, arrived at through means of the specialization of *nakshatra* as the distinctive name of the lunar asterisms, and then the use of these as marking and denominating the spaces in the sky which they occupied. The scholiast to the Sûrya-Siddhânta calls by the name *nakshatra* not the twenty-eight lunar asterisms alone, but also Sirius, Capella, and the other fixed stars whose positions are defined in connection with theirs (Sûrya-Siddh., viii. 10–12, 20–21); and the astronomical treatises themselves, when they wish to speak exactly, avoiding the ambiguity arising from the double use of the term, do so by setting up a new special name for the spaces, namely *nakshatra-bhoga*, *bhabhoga*, 'the portion of [the heavens, or of the planetary path,* belonging to] an asterism.' I submit, therefore, that when we would avoid the same ambiguity, we are fully justified—etymologically, scientifically, and by every other consideration—in talking of "asterisms" and "asterismal portions or spaces." I made choice of the word *asterism* because, while more usually employed to designate a group of stars, it does not etymologically or invariably do so, and is therefore conveniently applicable to the *nakshatras*, which, though prevailingly groups, count among their number a few single stars.

Prof. Weber's opinion and my own are still at variance—perhaps, after all, less in reality than in appearance—as to what constitutes the central and fundamental feature of the asterismal system, and what was its history of growth. He holds (p. 436) that "the division of the heaven into twenty-seven portions, corresponding with the daily motion of the moon," was the earlier step, and that "the selection of the stars which should mark those portions" was the later step. This is, if I am not mistaken, partly right, but also partly wrong and wanting in coherence. The division of the heavens may well enough be claimed to have been the first thing accomplished; but how, I would ask, could it be accomplished save through the means of the selection of stars? Prof. Weber will hardly be disposed to maintain that the Hindus can have first staked out or chalked off the sky into twenty-seven sections of 13° 20′ each, and then proceeded to examine each section

* I ought, perhaps, to shun the use of this phrase, as Prof. Weber (p. 436) suffers himself to be misled by my innocently introducing it here and there as synonym for 'ecliptic,' to avoid the burdensome repetition of the latter and its other equivalents, into suspecting me of heresy and inconsistency touching the Hindu knowledge of the planets.

and see which among the stars it contained could be most conveniently employed to mark it? Any assumption of an independent division, made by the aid of a clock and meridian circle, or of any other astronomical instruments, is excluded by the nature of the case.* Weber's earlier step, then, would be no step at all, unless combined with his later one. It would issue merely in the formation of an idea of a division, in a preliminary apprehension that the heavens might, could, would, and should be parted off into twenty-seven or twenty-eight portions, each of which was traversed by the moon in a day's motion. Such an apprehension is, indeed, an essential prerequisite to the establishment of a system, but it is nothing more than that; it might come to be realized in the sky, and it might not; it is a mere idea, and not an institution. The institution is founded when the division is actually made; that is to say, when the determining stars and groups of stars are selected, assigned to their purpose, and combined into a series. In performing this work, we cannot well conceive that anything but the moon's motion itself served as guide; observation of her position from night to night, and through a number of successive revolutions—with some aid, possibly enough, from the determination of opposite groups, as shown by their position upon opposite horizons, or from other such simple methods—would lead gradually to the choice of the asterisms, and the formation of a satisfactory series, answering thenceforth (to adopt Müller's very apt comparison) the purpose of the figures on the dial-plate, to mark the point in her revolution at which the moon had at any given time arrived. What a dial-plate is without figures and without fixed position, that would be a lunar zodiac without designated asterisms. In the series of asterisms, and the system of divisions as bound to it and dependent on it, we have a real institution, capable of being described, handed down by tradition, communicated to other peoples. And the asterisms are the visible and concrete portion of the institution, that which determines its identity, that to which the tradition would cling most closely; that they should be loosely held, or lightly abandoned, and the original work of selection done over again in any wholesale way, is not a thing to be thought of. Some degree of modification it would, of course, like every other human institution, be liable to undergo; there is not one of the groups composing it which might not be changed for another if continued use should show

* Unless, indeed, we are to adopt Müller's acute suggestion (Preface to the fourth volume of the Rig-Veda, p. lii.), that "any twenty-seven poles planted in a circle at equal distances round a house would answer the purpose of a primitive observatory" for the *nakshatras* as portions of the sky, and for the positions of the sun and moon among them. The apparatus would, no doubt, answer such a purpose famously, with two slight modifications: in the first place, instead of a house among the poles, we should need a twenty-eighth pole at their centre, to the apex of which the observer might apply his eye, while he revolved about it; and, in the second place, we should have to reduce the ecliptic to a coincidence with the horizon—and this would be harder to manage: for it would imply the transfer of our observatory to a place about 67° north or south of the equator; and, even there, the coincidence could happen but once a day, when the opposite solstice was under our meridian. After all, it is hardly true that "our notions of astronomy cannot be too crude and imperfect if we wish to understand the first beginnings in the reckonings of days, and seasons, and years" (ibid.).

that the substitution would be an improvement, or if other considerations should prompt it; which of the series should be accounted as the first would be (in the absence of any determining motive directly connected with the moon and her motions) a matter of comparative indifference, to be settled by changing usage. In short, every variation which we actually note in the three systems, upon comparing them with one another, is such as we might look for, being consistent with all that strictness of tradition which we have reason to expect in an institution of this character. Even the complete change of application to which the Chinese have submitted it is but the natural accompaniment of a change in their general methods of astronomical study and observation, the complete carrying out, as I have already indicated (above, p. 44), of a process which the Hindus also initiated, but pursued no farther.

Somewhat farther on (p. 438 sq.), Prof. Weber seems desirous to fasten upon me the reproach of disingenuousness (not to call it by a worse name), in that I have, under the influence of his Essays, changed my views in one important point respecting the *nakshatras*, without acknowledging it, and even with an attempt to hide the fact from sight. In the notes to the Sûrya-Siddhânta, namely (pp. 207–8; Journ. Am. Or. Soc'y, vi. 351–2), I had at some length argued that the Hindu *nakshatras* were not properly to be regarded as a lunar series; that they were, rather, a simple system of ecliptic division, made for general uses: while, in my article on the asterismal system, that opinion was tacitly abandoned, passed without a mention. Owing to the peculiar circumstances under which our Sûrya-Siddhânta was prepared and published —I having to do the bulk of my investigation of each separate subject as it came up in order, and being unable to detain the printer until I should arrive everywhere at a fully matured conclusion—the notes contain more than one opinion which I have since seen reason to relinquish as untenable. Some of my errors I discovered in time to rectify them, partially or completely, in the "additional notes;" others (of which by far the most important is my provisional adhesion to M. Biot's views respecting the derivation of the *nakshatras* from the *sieu*) I have corrected elsewhere, or should do so in a second edition of the work, or at any other suitable opportunity. Of the former class is the view now under consideration; I had arrived at it under the joint influence of Biot's teachings and of the Sûrya-Siddhânta, in which treatise the *nakshatras* exhibit no trace of a special connection with the moon; but though I held it at the moment with considerable confidence, I soon saw reason seriously to question its correctness; and I withdrew it, virtually, if not categorically, in the twenty-eighth additional note (p. 325; Journ. Am. Or. Soc'y, vi. 469), where I come to the contrary conclusion—namely that "it may fairly be claimed that the asterisms, as a Hindu institution, are an originally lunar division of the zodiac." My change of opinion was due to a renewed and fuller consideration of the same evidence which I had had before me in already discussing the question, and also to my growing emancipation from subservience to Biot's dogmas. That Prof. Weber's writings and private communications had an important share in enlightening me I have no disposition to question, although I can no longer recall all the steps of my conversion: but he is altogether mis-

taken in supposing that his Essays on the Nakshatras were instrumental in effecting it; when they appeared, the question was already a thing of the past to me; so little was it present to my mind, and so far was I from thinking that, after what I had said in the additional note, he would still regard me as maintaining the opinion put forth and defended in the notes on the text,* that (as he truly points out on p. 438) I totally misapprehended the meaning of the remark made by him in his first Essay (p. 316), that he should prove erroneous my assumption that the *nakshatras* were zodiacal constellations, rudely marking out divisions of the ecliptic. I think, therefore, that Weber judges me with unnecessary harshness, or even with evident injustice, when he holds up to reprehension the claim made in my article, that my view of the asterisms still remained, "in *nearly* all essential respects" (namely, in those which I go on to detail—among them, that the series was looked upon as having been selected to mark the moon's progress through the zodiac) "the same with that expressed in the notes on the Sûrya-Siddhânta," and when he would fain show that, in examining and criticizing his Essays, I have intentionally ignored one point of prime consequence respecting which I had been convinced by their arguments. I may be unreasonably difficult to convince of the erroneousness of an opinion which I entertain: of that others must be the judges: but I believe that I know myself well enough to be justified in claiming that I am ever ready, when convinced, to confess it with frankness.

To my former discussion (above, p. 56 sq.) of the question whether the Hindu asterisms are originally twenty-seven or twenty-eight I have nothing of consequence now to add, by way either of extension or of amendment: my argument was drawn out with the most scrupulous care to allow no fact or consideration more weight than fairly belonged to it, and I am unable at present to see that it requires modification. Prof. Weber's criticisms do not touch it: he has failed to apprehend my position, as a purely negative one; to see that I am not trying to prove the number twenty-eight original by Indian evidence—which, indeed, I should be presumptuous to attempt—but only to show that that evidence does not unequivocally prove the number twenty-seven original; that, if the primitiveness of the series of twenty-eight is strongly supported by evidence obtainable outside of India, it cannot be confuted from the Hindu literature. Weber, in his partizanship for the contrary opinion, would fain compel me to infer that the seventh hymn in the nineteenth book of the Atharva-Veda, in which Abhijit is mentioned along with the other twenty-seven asterisms, must necessarily be a very modern one, because the book which contains it is a late addition to the Atharvan. But the demand is obviously unreasonable and groundless. The same nineteenth book includes passages, from single verses to whole hymns, which are also found scattered through five of the ten books of the Rig-Veda: and Weber would hardly require us to admit that half the Rig-Veda is more recent than the main body of the Atharvan collection;

* It was doubtless the case that he simply overlooked the additional note, as was so easy to happen, although it is fully referred to in the index, under its proper subjects.

yet, if we must allow that the seventh hymn, because of the place where it occurs, cannot be otherwise than modern, then our conclusion must be the same as regards the tenth and eleventh (which are Rig-Veda vii. 35), or the sixth and thirteenth (which are Rig-Veda x. 90 and 103), and so on. As for the palpably recent words, to which Prof. Weber a second time refers us as indicating the late date of the hymn in question, they do not happen to be contained in the hymn itself, but in another one, an appendage to it, of very different style and content.

In weighing and combining such doubtful and (apparently) partially conflicting evidences as we have to rely upon in making up our view of this general subject, one person will naturally attribute more force to considerations of one kind, another of another. To myself, I will own, almost anything seems easier to assume than the repeated borrowings, the successive alterations under foreign influence, to which Prof. Weber is so ready to resort as a solution of the difficulties in which his reasonings involve him. He thinks that the Hindus may have first got a series of twenty-seven asterisms from Babylon, and then, afterward, another of twenty-eight, extending the system because they understood that the Babylonians had done the same; that the Arabs may have accepted from the Babylonians, or perhaps from the Hindus, in ante-Islamic times, an asterismal system, which they certainly cast away, putting in its place a modern Hindu one, at some later period; that the Chinese *sieu*, too, are of Babylonian origin, but that their partial agreement with the *nakshatras* of the Sûrya-Siddhânta is a mysterious circumstance, to be conjecturally explained by communication, in the one direction or the other. In my view, on the other hand, an institution of this character, when once introduced and naturalized, fairly adopted by the people to whom it comes and made familiar to their use, is thenceforth virtually a native institution, having its history of development determined by internal circumstances, not readily changed from a proneness to imitate what is foreign. The grand reason for believing that the Hindu system is originally composed of twenty-eight members is that both the others are so; and, to my apprehension, it outweighs all the apparently opposing circumstances, and forces us to endeavor to explain these in such a manner as to accord with it. In fact, the more clearly Prof. Weber is able to show that in the early times, no less than in the later, the Hindus prevailingly reckoned twenty-seven *nakshatras*, the more does he increase the probability that they began with reckoning twenty-eight; inasmuch as he thereby decreases the probability that the *sieu* and *manâzil*, which are systems of twenty-eight members, were derived from the *nakshatras*, and forces us toward the conclusion that they all originate together from a fourth source, or else that the *nakshatras* are derived from the *sieu* or the *manâzil*. In the Siddhântas, the Jyotisha, and the Brâhmaṇas, the division of the ecliptic is twenty-seven-fold, and the twenty-eighth asterism is a *hors d'œuvre*. The most conspicuous result of Steinschneider's recent inquiries among the Arab authors, according to his own (provisional) summary (see above, p. 383, note), is to the effect that they report the Hindu astronomers to reckon twenty-seven *nakshatras*. The consideration, then, that, if the Hindus had propagated a lunar zodiac through Asia, they would

have been likely to propagate one of twenty-seven divisions, and not of twenty-eight, is perhaps entitled to even more weight in the general argument than I formerly (above, p. 57) claimed in its behalf.

Prof. Weber simply scouts as impossible my opinion that the words *uparishṭât* and *avastât*, literally 'above' and 'below,' used by the Tâittirîya-Brâhmaṇa in describing the position of Abhijit relatively to the Ashâḍhâs and Çroṇâ, *may* be rendered 'beyond' and 'this side,' and understood to designate the rank of Abhijit in the series rather than its position among the stars: he insists that they can only signify 'farther up in the sky' and 'farther down in the sky.' This seems not altogether consistent with the position which he formerly maintained, in his controversy with Goldstücker respecting the antiquity of the art of writing in India. Whereas the latter asserted that the words "above" and "below," as employed by the Hindu writers (in a manner precisely the opposite of that usual among us) to signify respectively a succeeding and a preceding passage, were to be taken in a perfectly literal sense, as indicating physical position in the pile of written leaves constituting a manuscript, Weber (Ind. Stud., v. 33) claimed for them a figurative application merely, and pointed out that various Sanskrit words meaning 'above' were frequently used in the sense of 'farther on, subsequent,' in connections where no conception of physical position was assumable. If, then, expressions for 'above' are familiarly employed by the Hindus to mean 'beyond,' without reference to physical position, I see no absurdity, nor even any special difficulty, in so understanding *uparishṭât* in the passage under discussion, even though I am unable to put my finger upon another passage where it has this signification: the two ideas of 'above in place' and 'beyond in order' are so convertible in ordinary Sanskrit usage, that any word which literally means the one may be employed to mean the other. The correlation of *uparishṭât* to *avastât*, which confessedly admits the contrary sense, of 'hither, this side,' would of itself be enough to create the possibility. And a possibility is all that I require or claim; my persuasion that Abhijit has not changed its stellar place during the period of our knowledge of the Hindu system is founded on other considerations, and is strong enough to overbear the presumption which this passage would otherwise, no doubt, more naturally raise.

In the same passage I can see no necessary implication, such as is claimed over and over again by Prof. Weber, of the *newness* of Abhijit as a member of the asterismal system. It is, indeed, spoken of as if it required something more than a simple mention, like the rest; but that might be not only because it was of recent introduction, but because it was not generally accepted as included in the series of *nakshatras* along with the other twenty-seven, and was therefore less familiarly known. And if it had been fairly brought in, since the time of the Tâittirîya-Saṃhitâ, into the system, and now formed an integral part of it, as recognized by the Brâhmaṇa, we should hardly find that this work, in two out of the five passages where the *nakshatras* are mentioned, rehearses only twenty-seven of them. Precisely this equivocal position of Abhijit, now acknowledged and now rejected by the same authorities, throughout nearly the whole period of subsistence of the institution in India, is what gives

support to my conjecture concerning it, that it is and was always a sort of outside member, with only a half-right to association with the rest.

That Abhijit has changed its place in the sky without changing its name is not a fact which Prof. Weber ought on all accounts to desire to insist upon, since its establishment tends to invalidate the claim that change of place and change of name go hand in hand with one another —a claim which he advances and urges, in order to prove the shifting and variable character of the *nakshatra*-series. Nor is the claim better supported upon the other side. Among all the variations of name presented by the authorities of various periods, we have none more striking than *mṛgaçiras* and *invakâs*, *ârdrâ* and *bâhu*, *mûla* and *vicṛtâu*: but, as regards the first of these, while we have stronger and more unequivocal reasons to believe in the ever unchanged identity of the asterism than is the case, perhaps, anywhere else in the series, we have also the assurance of the highest Hindu lexicographic authority that the two names are of identical meaning;* in the second case, the star called usually *ârdrâ* marks the *bâhu*, or 'fore-leg,' of the "Stag;" in the third case, al-Biruni reports the asterism to have two stars, as indicated by the dual *vicṛtâu*, and identifies it with that pair which forms the most brilliant and conspicuous feature of the larger group styled by the Siddhântas *mûla*. In view of these and other like facts, I may safely appeal to any unprejudiced person whether variation of name is to be taken as *primâ facie* evidence of change of place of an asterism.

If the Kâṭhaka, by reason of the circumstance that men in general are not versed in the intricacies of the astronomical and astrological doctrine of the *nakshatras*, is willing to leave it sometimes to the option of the individual sacrificer whether he will take any account of them or no, such conduct is very liberal on its part, and much to its credit as a spiritual guide; but will hardly conduct us, as Weber (p. 454) would have it do, to the conclusion that the whole subject was of such obscurity that even the astrologers could not feel certain which groups of

* It is not a little strange that even here, where the traditions of the Brâhmaṇas and the astronomical data of the Siddhântas agree in fixing the place of the asterism beyond dispute, and where there is no shadow of a reason discoverable for our believing it to have changed its identity in the interval, Weber is still ready (p. 452) to assume that the Amarakosha's identification of *mṛgaçiras* and *invakâs* is only an inferential blunder. This is quite of a piece with his former suggestion of a merely etymological reason for the definition of *mṛgaçiras* as a group of three stars —a suggestion upon which I especially animadverted in my previous paper (above, pp. 52-3). Prof. Weber appears to think at present (p. 452) that he should have been shielded from any reproach on this account by the fact that he had himself extracted and brought together the legends and other evidences which fix so distinctly the identity of the group. But, in my view, the case is just the other way. If he had not known the evidences, his suggestion would have been a less unnatural one, and simply refutable by their adduction; the very circumstance that, while having them all in view, he could yet hazard a conjecture which wholly ignored them, was what appeared to me so unaccountable that I could only compare it with the effects of a wilful blindness. I sincerely regret to see that the strength of my expressions in connection with this subject has wounded Weber, making him regard himself as accused of deliberately shutting his eyes to the truth: nothing was farther from my thoughts; I supposed that I had sufficiently guarded against such a misconstruction by what I had said on the preceding page (p. 51), as well as earlier (p. 10).

stars constituted the series, or hand down the knowledge of them unimpaired from generation to generation. On the contrary, the first great blow to the exact tradition of the system considered as a stellar one was given, in my opinion, when the Hindus were turned from rude observers into exact calculators; when the precise data and methods of their borrowed astronomical science sent the *savants* of India to their closets, instead of to the open fields, as the scene of their learned labors; when the *nakshatra* with which the moon should be found at any particular time could be correctly determined by one who never looked at the sky, and was unable to tell one star from another. Then began the period when even he who was most versed in the *nakshatra* doctrine might at the same time be utterly indifferent as to which were the stars and the groups whose names he used so glibly. Hence the difficulty experienced by al-Biruni (at the time of whose visit to India this had already been the condition of things for centuries), and by all who have followed him, in making the Hindu astronomers point out their asterisms in the sky. Hence also, as a last example, Bâpû-Deva Çâstrin, one of the most learned and able of the living Hindu votaries of the science, in his translation of the Sûrya-Siddhânta (Calcutta, 1862; p. 62), makes no pretense to an independent opinion as to the identity of the asterismal groups, but adopts implicitly Colebrooke's determination of them, not venturing to vary in the slightest particular from his authority, even where it is most obviously mistaken.*

Nearly at the close of his essay, Prof. Weber, if I rightly apprehend his meaning, plants himself on ground where I can heartily join him, and where, as I think, most of our conflicting views may be harmonized. He points out, namely (p. 454), that, considering the lateness of the Siddhântas as sources of our knowledge respecting the *nakshatras*, we have reason even to be astonished† that the positions of the latter, as determined by them, agree so well as they do with those derived from the Arab and Chinese authorities; and he adds: "But this agreement is naturally the best guarantee of their correctness, and, where it is found to exist, the identity of the stars concerned is, naturally, assured. Where, however, there is no such agreement—where, that is to say, the *manâzil* and *sieu* correspond, but the *nakshatras* differ from them—there the fault will doubtless lie with the latter, being attributable to their defective tradition."

This is precisely the position which I would desire to maintain. If

* It is important that the utter subserviency of Bâpû-Deva in this matter should be remarked (the more especially, since it is unacknowledged, no reference whatever being made to any authority), lest it be supposed that he intelligently accepts and ratifies Colebrooke's conclusions, as agreeing with the results of his own examination of the matters concerned. For example, even Colebrooke's identification of Apâmvatsa with "b 1, 2, 3" in Virgo is copied, although, as I have already once pointed out (Sûrya-Siddh., p. 219; Journ. Am. Or. Soc'y, vi. 363), and have since verified by reference to eminent astronomical authority, there are no stars known to science by those names.

† That is to say, of course, if we have suffered ourselves to be persuaded by Prof. Weber's arguments that the asterismal groups were liable to and actually did suffer indefinite change, and that the Hindu astronomers were never quite assured of their identity: I know of no other ground for astonishment.

we are to investigate the history and relations of the three systems, or of any one of them, our first step, the foundation of all our after conclusions, must be their mutual comparison: the results to be derived therefrom are surer and more reliable than any which we can obtain by other merely inferential means. Where the three are found to agree, there no theoretic considerations of general variability, of changing names, divinities, or numbers, or the like, will justify us in assuming that any one of them has deviated from the original; nothing but positive and unequivocal testimony can show such deviation. Where any two of the three agree, and the third differs from them, we have at least *primâ facie* reason to believe that the former truly present the primitive institution, which the other has at this point abandoned—for what reasons, and at what period, must be judged in each case separately, upon testimony or from conjecture. In the very few cases (not more than two or three) where all disagree, only conjecture, guided by considerations of general fitness, can presume to point out the original. Such a comparison as this I have attempted to make, in my former article (at page 45), and I claim with confidence, now as then, that it should constitute the basis of every general inquiry into the origin and transmission of this interesting and problematical institution. Those who thus begin alike will be likely to agree in their main results, however they may differ in regard to details. Different minds are differently impressed by the same evidence, and what is satisfactory ground for a decided opinion to one person only suggests a presumption, if even that, to another. For my own part, the sole opinion which I can hold with confidence is that every attempt hitherto made to prove any one of the three systems derived from either of the others is demonstrably a failure; but, partly for that reason, partly for others already set forth, I incline to think, with Prof. Weber, that some fourth people is most likely to have been the originator of the primitive lunar zodiac.

It remains to say a few words upon the same two points of which I spoke by way of appendix to my first article; since, though they are not immediately connected with the question of derivation of the *nakshatras*, their treatment by Prof. Weber is not without an important bearing upon the controversy between us.

To one accustomed to deal with mathematical questions, the simple statement that the moon's synodical revolution and the yearly revolution of the sun are entirely incommensurable—that the year is composed, not of twelve lunar months, nor of twelve *plus* or *minus* a minute fraction, but of about twelve and a third—is sufficient foundation for the inference that full moon must occur in all parts and at all points of the ecliptic, and could not be bound for any continuous period to any particular series of parts or points. For the benefit, however, of those to whom the bearings of such a statement would be less clear, and quite especially for the benefit of Prof. Weber, who had already shown that he did not appreciate them, I spent a good many hours, while preparing my previous essay, in calculating the sidereal place of the moon's opposition through a series of years, in order to show by a sufficient illustrative example precisely what would be the effect of the incommensurability referred to in shifting the places of the moon's full from

asterism to asterism. And I submit at present that the illustration *was* both clear and sufficient, and ought to convince any well-informed and unprejudiced person. So far, however, as Prof. Weber is concerned, my labor was thrown away: he is still quite incredulous, and anxious to have the matter tested anew, "by competent astronomical authority." What authority he may be willing to accept as competent, it is impossible for me to say; but I cannot see that any profound astronomical science, that much more than a little arithmetic, is called for in order to deal with a question so simple. He would surely have done both himself and me better justice, if, instead of publishing to the world his own inability to judge the case and his lack of confidence in me, he had referred my assertions to some mathematical friend to be tested. The difficulty is that, having formed a very plausible theory in explanation of a historical fact, he is unwilling to see that it involves an impossibility, and must therefore be abandoned. It appears to him so "decidedly" and "in the nature of things" to be taken for granted that, if the Hindus have a set of permanent names for the months derived from certain *nakshatras*, they must have selected them because, at the time, the moon was continuously or customarily full in those *nakshatras* rather than in any others of the series, that he is unreasonably impenetrable to the proof that this never was or could have been the case. To me, on the other hand, it seems not hard to find an explanation which shall include and reconcile both the historical fact and the astronomical. It was practically more convenient for the Hindus to have fixed names for their months, rather than such as varied from year to year, according to the asterisms in which the moon was actually full; and the present series was pitched upon simply because it was as good as any other. The work may even have been done in the closet, without reference to its real truth in any one year. It was necessary to make a somewhat arbitrary choice, and the Hindus had sufficient good sense to make it, and to establish a fixed nomenclature for their months, without being disturbed by the fact that it could never be strictly correct for two years in succession.

The other case is of a very similar character. In his Essays on the Nakshatras, Prof. Weber points out that certain older Hindu authorities treat Phâlguna as first of the spring months, while certain later ones put Câitra in its place; and yet others, whom he regards as the latest of all, allow the same rank to Vâiçâkha; and he suggests that the precession of the equinoxes furnishes a sufficient explanation of the substitution. I, in my criticism, reply that in this last point he is mistaken; that he must seek some other mode of accounting for the discordance of his authorities, since, so far as the precession is concerned, we should expect just the contrary to be the case. Now, however, he insists, on philological grounds, upon his former explanation, thus converting what was at first a venial oversight into a discreditable blunder. He combats my objections as if I were trying to force him to admit that *sûtras* are two thousand years older than *brâhmaṇas*, and comments than *sûtras*. I answer, as in the preceding instance, that this is not a matter with which I have undertaken to meddle; that I am not opposing his philological facts, but only putting alongside them another, of a

scientific character, which overthrows, not those facts themselves, but his explanation of them, compelling him to seek another—which need not be difficult to find. A very slight consideration of the point, with the aid of a globe or chart, or of consultation with some friend better versed in such subjects, might have shown him his error, and saved him from putting himself into the somewhat equivocal position of one who attempts to prove, "as philologue," that the precessional movement of the equinoxes is from west to east, and not from east to west.

These two instances, in which Prof. Weber discredits and rejects, without any due examination, my well-meant rectifications of his conclusions, in matters respecting which he has not generally been unwilling to allow that I am better qualified to judge than he, seem to me to indicate that he has put himself into a more than reasonably antagonistic and repellent attitude toward my article, and all its reasonings and conclusions. That it was so may doubtless be ascribed in great part to the effect of those misapprehensions of his position on one or two important points which I have above had to excuse or explain. I am not without hope that at present, in view of my explanations, he will be led to reopen the discussion in his own mind, and to attribute more weight than he has hitherto been able to do to my views upon the points as to which we differ.